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THE AMERICAN
METEOROLOGICAL JOURNAL.

VOL. VIII. ANN ARBOR, MICH., JUNE, 1891. No. 2.

ORIGINAL ARTICLES.

NEW ENGLAND METEOROLOGICAL SOCIETY.

The twenty-first regular meeting was held at Newburyport, Mass., April 18, 1891. The subject for discussion was "Weather Predicting." The general methods of predicting in the United States and Europe were first described and afterwards local and long-range predictions were considered in the following papers:

THE U. S. SIGNAL SERVICE WEATHER FORECASTS.

BY J. WARREN SMITH OF CAMBRIDGE.

Weather forecasting cannot be done from the data at one point of observation. It is only when we can have the results from a great number of simultaneous observations covering a large territory that one can pretend to forecast with any degree of accuracy. This was well understood when the Signal Service weather work was first organized, and the first reports were from systematized synchronous observations transmitted by telegraph in 1870. The system embraced twenty-four stations and observer—sergeants, and with these observations began the first practical workings of the Signal Service weather work in this country. Since then the hours of observation have changed and the number of observations has been reduced as self-registering instruments have obviated the necessity of frequent eye observations; the manner of putting the information before the public has improved from the rude bulletin to the fine lithographic map at Washington, and the improved

milliographic map at the sub-stations; but the elements of observation and the whole general system remain practically the same.

At the present time there are about 150 stations in the United States sending telegraphic reports twice daily to the Chief Office; 26 in Canada and one at Havana, Cuba. The regular observations are taken at 8 A. M. and 8 P. M., 75th meridian time. The elements of observation are air pressure, temperature, dew point, relative humidity, precipitation, direction and force of wind and the amount, kind and direction of clouds. As the temperature is considered one of the most important and changeable of these, the observer is instructed to begin observations so that he shall be reading the thermometer at exactly eight o'clock. If you could have taken a bird's-eye view of the United States this morning at about two minutes of eight o'clock, you would have seen observers all over the country climbing the stairs to their instrument shelters.

It is necessary that the results of these observations shall be in the Chief Office as promptly as possible; at twenty minutes past eight the telegram should be at the telegraph office ready for transmittal, and when there it takes precedence over every other message, government or otherwise, except in time of war. The telegraphic report is in the form of a cipher, not for secrecy, but to save expense and time; by an ingenious arrangement of vowels and consonants the message is sent in a much less number of words than could be done with figures, and is as easily read by a skilled observer. A telegram sent from the Boston office on the evening of April 3, read "Boston Fanfoot Regnest Beget Holdfast Moral Perfidy Null." Deciphered in regular order it would read, "Boston, current barometer, 29.32 inches; current temperature, 38; wind from the west; weather, cloudy; precipitation in past twelve hours, 0.01 inch; current wind 14 miles per hour; maximum temperature during the day, 44°; observers' local prediction for the following 24 hours, fair; dew point, 32°; sky entirely covered with stratus clouds moving from the west; maximum wind during the past 24 hours, 36 miles an hour from the west; the precipitation for the week ending on that evening was just equal to the normal, and the temperature for the week was 4 degrees below the normal."

As fast as the telegrams are received at the Chief Signal Office they are deciphered by an expert, and five charts are constructed by assistants for the use of the forecasting official.

These charts are 1st, weather chart; 2d, pressure changes; 3d, temperature changes; 4th, clouds and maximum or minimum temperatures and changes; 5th, dew point.

Chart I (weather) shows for each station; 1st, temperature; 2d, pressure reduced to sea level; 3d, wind velocity, and, when reported, the maximum velocity since last regular report; 4th, the precipitation; 5th, the state of weather; 6th, wind direction. Isotherms are drawn in blue for each ten degrees of temperature, and isobars are drawn in red for each tenth of an inch of barometric pressure. The position of the storm centre at the hour of observation is indicated and the movement of the storm is shown.

Chart II (pressure changes) shows the change in the pressure for twelve hours and twenty-four hours, and the departure from the normal.

Chart III (temperature changes) shows the temperature change in twelve hours with the diurnal change eliminated, the change in twenty-four hours and the departure from the normal.

Chart IV (clouds) shows the maximum and minimum temperature and changes in the same and, by means of symbols, the kinds, amounts and directions of the clouds. The maximum and minimum temperatures are not charted for forecasting purposes, but only to show the extremes, as the changes correspond closely to those of the current temperatures.

Chart V is the dewpoint chart and is prepared under the direction of each Forecast Official in accordance with his own plan, one prefering it to show simply the twelve and twenty-four hour changes in dewpoints, another the depression of the dewpoint and twenty-four hour changes, and another the twenty-four hour changes and departures from the normal. All the Forecast Officials agree in regarding Chart I as by far the most valuable and indeed the main one of the series as an aid to forecasting. As to the value of the others they are not agreed, one official deriving, as he thinks, considerable aid from a chart that another seldom, if ever, consults. They are generally regarded, at the most, as helps in the saving of time, as nearly all the information found in them can be obtained from the study of Chart I. The forecasts as soon as formulated are telegraphed to Signal Service stations and section centers, and to the Associated Press, and from the section centers they are sent by the observers in charge to the sub-stations and display-men.

This Society occasionally receives letters from the observers commenting on some mistake that the Forecasting Officer has made and giving the forecasts as they would have made them; but it is plainly seen that these persons do not realize the difference between studying the map at their leisure when the period for which the weather is forecasted is half over, and forecasting from the original charts for many definite sections, the weather and temperature, for a definite period of time, and doing this quickly and finally. They do not understand the enormous amount of work devolving upon a single official in a very brief time. The forecasts to be of value must reach the Associated Press, for distribution to the newspapers and thus to the people, within a very few minutes after the completion of the map. As a general rule, only a fraction of a minute is available in which the official can formulate his opinion and dictate the forecasts for any particular state or section under consideration.

It would seem that this section of New England is already blessed (or cursed), with plenty of weather forecasting, for with the Chief Signal Office forecasts, the local forecasts made by the observer of the Boston Signal Office, those made at Blue Hill, and the half dozen newspapers which give special forecasts, one can almost select any kind of weather he wishes for. But there is probably not another section of our size in the world that will stand so much forecasting as will New England, for there is hardly another section that has so much and such a variety of weather. Nearly all the storms that come across the country pass off the coast across or near New England. Those which pass to the north of us give the warm, sultry, southerly winds, generally followed by clearing westerly winds and sometimes by equally northwest winds. The storms that pass to the south and up our coast give chilling northeasterly winds changing to westerly, and if it be in the winter season, accompanied by snows.

To one who takes no note of the cause of weather changes, our days must seem a conglomeration of good and bad weather; of rainy days and days of sunshine. But to the student of the weather all his heterogenous mass organizes itself into a succession of characteristic types. Our hot muggy days with southerly winds, most noticeable when they occur in winter, are weather types, as are also the squally northwest days which follow a cyclone. It seems that a scientific body like this Society

can find no more important duty than to investigate and systematize these types and to find out if possible under just what conditions they all occur.

We hear a great deal of talk as to the immediate improvements in the forecasts that we are going to get after the Weather Bureau is transferred to the Agricultural Department, but that is expecting too much. Of course, we may expect steady improvement in the future as there has been in the past. The thing to be hoped for is a wider and earlier dissemination as well as an improvement of the forecasts which we already have. We want to see them in every city, village and town in New England as early as they are now received at Boston. When this is accomplished, we shall see people depending more upon and being benefitted more by them.

EUROPEAN WEATHER PREDICTIONS.

By W. M. Davis.

Professor Davis spoke of the weather maps of Europe, illustrating his remarks with a series of foreign charts, lent by Mr. Rotch. Weather maps are essentially similar, the world over; they all represent by conventional signs the condition of the weather over a large area at a single time. They are, or try to be, synoptic. But in certain relatively subordinate ways, the European maps differ from ours. In the first place, they are not truly synoptic for the whole of the area that they represent. The international exchange of observations, by which the preparation of maps of a sufficiently large area is alone made possible, is not yet accompanied by a perfect reduction of all observations to a single time. With us, all observers go to the instrument shelters just before 8 A. M. and 8 P. M. of eastern time. In Europe, there is a certain diversity of the time of observation between the several countries, although the records are afterwards plotted as if truly synchronous; the error involved cannot be great, for the results obtained are generally satisfactory; but the inaccuracy of the method places it behind ours.

In the second place, the observations are not as systematically uniform as ours; for they are not taken under a single control, as they are here. In Europe, there is in the first place, a certain diversity of method in the services of the different countries

and even in the observers of the same service; for there it is not always the case that the time of the observers is entirely engaged by the service to which they report; they may be persons of regular occupations in responsible positions, but responsible first to other authority than the weather service. Although ideally inferior to the system in operation with us, the practical results of their method do not lead to complaint. Indeed, we might say that they show our method to be unnecessarily expensive; we might get a large number of additional records at small cost from observatories, universities and agricultural experiment stations, from observers whose salary would be paid in chief part by the institution to which they are attached.

In the third place, the gathering of the more remote observations is retarded in Europe by a want of an international control of the telegraph lines. With us, the weather observations have precedence of everything in time of peace; in Europe, no such promptness is attainable. It is surprising, however, how promptly the international exchanges are made, when the difficulties of their transmission are considered.

When we examine European maps, there are several marked differences from ours. In the first place, they are smaller, and often of less elegant appearance; and while this does not count for much in a scientific way, I think it is perhaps a difficulty in the way of their popular usefulness. There are no maps in the world of the elegance of those that have been issued by our Weather Bureau. Nearly all of the European maps are printed in a single color, and often in a rather untidy manner, from rough hand lithographic work. For that reason they are often more difficult to read than ours are. Many of them are made simpler than ours by the omission of isothermal lines; others, such as the German, contain a greater variety of weather indications, all represented by international symbols. Some present the isobars and winds on one map; and the isotherms and weather on another, both maps being on one sheet. Several bulletins indicate the condition of the sea at the ports by appropriate symbols; this might be added to our maps.

It is, however, when we come to the use of the maps in making predictions, that there is the strongest contrast between those in this country and in Europe. The areas for which the predictions are made are small; often so small that only one prediction need be made for the whole of it. Contrast this with the great number of districts into which our country must be

divided, and the excessive hurry with which our predictions are issued, when one officer has to make special predictions for many separate subdivisions. In Europe, the predictor may really take his time and make each prediction a matter of some deliberation. That is impossible with us; and therein lies the chief criticism to be made on our system as organized at present; it is too strongly centralized. It is not only that the predictions from Washington must be made hurriedly; but that one predictor must carry in his mind the weather characteristics of the whole country. Under such difficulties, it is hardly surprising that General Greely has to lament the scarcity of good predictors; the task imposed on them is too great. In Europe the task is easier; in Belgium, for example, the predicting officer has but a small territory to study; he must come to know it much more intimately than is possible for our officers to know our whole country.

I believe that this far more than makes up for the several slight inferiorities—ideal inferiorities, they might be called —that characterize the European maps. As a result of the concentration of attention and the leisure for study before predicting, we find more has been stated in the way of generalizations by the European predicting officers than by ours. With decentralization, and with the consequent greater time for study, I look for a greater share of results of this kind from our service. Indeed, it has always been a regret to me that we have not learned more from the officers of the Signal Service of the principles on which weather prediction is based. Their opportunity has been extraordinary, as far as materials are concerned; for many years we had maps prepared three times a day, a frequency greater than has been attempted anywhere else. Our area is so large that the map of our own service includes frequently enough entire individual cyclones or anticyclones; and added to this, we have the international maps, on which the relation of our cyclonic storms to the general movements of the atmosphere is given. There can be no doubt that much is yet to be learned in addition to the general statement that our storms move eastward at such and such a rate; but whether this is to be learned by a different use of observations that are now made by new observations, or in some other way, does not seem as yet to have been rigorously discussed. At least we have no formal report concerning the results of such an inquiry;

although the need of effort toward improvement is sufficiently demonstrated by the frequency of inaccurate predictions. We have not yet such a series of weather types as has been prepared by the officers of the German and other European services. It therefore seems to me that while the formal scheme of our service is better than theirs, yet we have not so fully taken advantage of our opportunities as they have of theirs. As already indicated above, the most apparent difficulty that we have suffered under is the centralization, by which one officer has been given too much predicting work to do in a very short time. Perhaps decentralization may cost more than now; but it might still be that the predictions would be cheaper; for a high price paid for predictions of moderately good quality is dearer than a higher price paid for better predictions.

The weather services of our own country and of Europe may be compared also with respect to the meteorological conditions of the two regions. We have the advantage again, if variety, suddenness of change, and intensity of development of weather elements may be called an advantage. There is no part of the world that has weather changes of the same rapidity, violence and variety as the northeastern part of our country; the part in which we dwell. This gives us more to study, and it undoubtedly increases the difficulty of the work. Europe is a region of relatively slow and moderate weather changes. We generally have notice of the storms that come upon us, except in the far western districts; while the storms that visit Europe come relatively unannounced from the Atlantic; but they move only a little more than half as fast as ours do, on the average, and thus give the predicting officer more time to announce them.

A test that is often resorted to in comparing the efficiency of weather services is the so-called percentage of success of their predictions. Unless quoted in the most guarded manner, there is nothing more deceptive than these percentages. I have not had and do not expect to have time to enter into this question far enough to learn how the percentages of success should be compared, but a little consideration will show that they are but seldom comparable. In the first place, if an observer on certain tropical coasts predicted fair weather all the year round, with on-shore winds by day and off-shore winds by night, he might gain a large percentage of success; in some months he might have no failures. But such success does not deserve the credit of a less complete success in this stormy country. The

relative rarity of frequency of the predicted phenomena therefore should enter the estimate of success; but how it should enter is a question for those versed in the theory of probabilities, as well as in the facts of meteorology. I believe that no account is taken of it in our tests. Secondly, the formulation of the predictions should be carefully looked after; for example, if "higher temperature" means only one degree higher as well as twenty degrees higher, if it includes one degree higher in one country, but requires a rise of at least five degrees in another, the predictions would not be comparable, although the words would be the same, the formulation is evidently vague. Precise definition of the words used is therefore necessary; and in order to warrant comparison the same words, similarly defined, should be used in the compared services.

Third, the area for which the prediction is made must be considered; the larger it is, the better the chance of success. Fourth, the time element is of extreme importance. Our predictions now cover a time which began before they are made; so that they are not altogether predictions. Other predictions sometimes cover a period that begins after the predictions are issued. The duration of the predicted period is also a matter of great importance; difficulty increases rapidly as the time increases.

Finally, the testing of predictions is no easy matter; its accuracy varies according to the number of stations in the area for which the predication was made; local rains, for example, need a full network of stations for their proper detection; general winds cannot be safely tested by sea-coast or valley stations; chinook stations do not give good means of testing general predictions of temperature. Is it therefore not safe to say that the mere numerical comparison of predictions in this country and in Europe, without adequate consideration of area, time, frequency of predicted elements, etc., is of little scientific value?

There is still another difference between our weather service and most of those of Europe; a difference that results from the unfortunate wording of the law with us, much more than from any other cause, as far as I can understand it; this is the absence of research in our service. It has not always been thus; under the greater part of General Hazen's administration, the "professional papers" of the service were serious attempts to improve our knowledge of meteorology, to utilize the great store of information gathered in the central office; and they sometimes

concerned not simply climatic data, but weather elements. This has very unfortunately been stopped, as being beyond the letter and intention of the law creating the weather service, and at present, our predicting officers are, as it were, ordered to predict but not to investigate. The archives of the German Seewarte, the memoirs of the Central Meteorological Bureau of France, the reports on thunder-storms issued by the Italian service, show that in the opinion of the legislative bodies of those countries the two parts of the duties of the officers in their meteorological services cannot be separated.

THE LOCAL WEATHER PREDICTIONS OF THE BLUE HILL OBSERVATORY.

By A. Lawrence Rotch.

One feature of the two most important European weather services, viz., Germany and France, is the decentralization of their predictions, by which, before the synoptic maps are issued from the national headquarters, a statement of the weather situation together with the intensity and position of the barometric maxima and minima are telegraphed in Germany from the national bureau in Hamburg to the dependent services in Bavaria, Saxony, etc., and to such private observatories in Prussia as those at Berlin, Magdeburg, and Cologne, where special predictions are made for these districts. In France, the general data are telegraphed at noon from Paris to the departmental observatories from which, in some cases, local predictions emanate.

This, therefore, is what is meant by local weather predictions, not predictions made from observations at a single station, but predictions made for that district from both general and local data. To illustrate my ideas I shall speak mainly of the work of the Blue Hill Observatory near Boston.

The Blue Hill Meteorological Observatory is on the highest land in Eastern Massachusetts and the view embraces one hundred and twenty towns and villages. Soon after its establishment, in 1885, its conspicuous situation was utilized to disseminate weather predictions over the neighboring country. These consisted of the cold wave warnings of the U. S. Signal Service and of local rain warnings. For the latter a red flag was displayed by day and a red lantern at night, whenever rain was expected in the vicinity within twelve hours' time.

Local predictions were facilitated after May 1, 1886, by the

charting of a daily weather map in Boston, showing the meteorological conditions existing in the United States at 7 A. M., 75th meridian time. Hitherto a map had been lithographed only in Washington and as it is received in Boston thirty-six hours after the observations which it contains are made, it is useless for predicting purposes. The issue of the map in Boston was due to Serg. O. B. Cole, then in charge of the Boston Signal Office, who obtained permission from the Chief Signal Officer to chart the telegraphic reports, which had been received for many years in Boston and had been given to the public in tabular form, and to manifold this chart by the cyclostyle process. This map proved so satisfactory that the tabular bulletin was discontinued and the maps substituted at all important stations, and I would call attention to the fact that the credit for the scheme belongs to Mr. Cole, the crude map printed in Boston, May 1, 1886, by Mr. Cole and the writer being the prototype of the elaborate weather maps which are now issued in large numbers from many Signal Service stations throughout the United States.

The first systematic local weather predictions in this country were probably the printed weather bulletins sent out by Prof. Cleveland Abbe every afternoon from the Cincinnati Observatory. Soon after commencing in September, 1869, telegraphic reports were received from fifteen stations and on October 6, predictions for Cincinnati and vicinity were commenced. This work has been continued by the United States Signal Service on a great scale with Washington as the central station, where, until recently, all the forecasts for the country east of the Rocky Mountains were made. A decentralization of the Signal Service forecasts was advocated by the writer in this JOURNAL for February, 1887, and the advantages of local forecasts, such as those which were being made at the Blue Hill Observatory, was pointed out.

This system, which was inaugurated July 1, 1886, and still in use with some modification, is as follows: Predictions of the weather and temperature for the twenty-four hours commencing at midnight are made about two o'clock of the preceding afternoon. The data on which they are based are the 8 A. M. reports from about one hundred and twenty United States and Canadian stations of pressure, temperature, wind, weather and precipitation, charted at the Boston Signal Station and received at the Blue Hill Observatory by messenger about 1 P. M., together with

a similar chart containing these elements for eight o'clock of the preceding evening. The local observations, including the traces of a number of automatic self-recording instruments, are also available up to the time of issuing the prediction, which is made five hours later and extends sixteen hours longer than the corresponding Signal Service forecast that is published in the same newspapers. The Blue Hill predictions have been made by Mr. H. Helm Clayton, and it is to his skill, acquired by a study of the movements of storm areas with respect to other areas of low and high pressure which affect New England, as seen on the synoptic charts, supplemented by the local indications, especially those furnished by the cloud movements, that their success is due.

The predictions are communicated to the public by means of flags and through the Associated Press. In the former code only rain or snow and cold waves are signaled. A red flag indicates rain or snow while no flag indicates fair weather. A white flag with a black center points to a decided fall of temperature below 45°, this being the cold wave signal of the Government Signal Service. The complete weather and temperature predictions for the twenty-four hours commencing at midnight are telephoned to the Boston office of the Associated Press by which they are sent to all the evening newspapers of Boston and to some papers in other cities. Special predictions have also been regularly furnished to private companies in Boston.

That "comparisons are odious" seems to be especially true of weather predictions, judging from the sharp controversy which arose when the subject of local vs. general weather predictions was mentioned in this JOURNAL. Numerical comparisons of different weather predictions are certainly meaningless if the methods of verification are not rigorously similar, and even then, as the chance occurrence of different kinds of weather varies with the locality, the relative skill of the predictor is not shown by the usual methods of estimating the success of the prediction. However these figures of verification may be interpreted, the object in instituting the Blue Hill predictions, which was by showing practically the superiority of the local over the general weather predictions, to prove to the Signal Service by an experiment at no cost to itself the advisability of decentralizing its Washington forecasts by having the district forecasts made at the chief station of each district by a competent person from the local observations and from the data of the synoptic chart,

has been accomplished. Local forecasts for Boston and vicinity formulated since August, 1889, by Serg. J. W. Smith, in charge of the Boston Signal Office have proved satisfactory and the same plan is now followed in other cities.

The following suggestions are made for the improvement of local weather predictions. The system of observing stations should report to a central bureau where the telegrams should be collated and a general summary of the situation prepared. This, together with the data necessary to construct synoptic charts, should be telegraphed to the district centers where the local forecasts would be prepared. It is important that a number of high-level stations, with corresponding low-level stations near their bases, should be included among the reporting stations since disturbances in the equilibrium of the atmosphere will often be shown by abnormal differences between the base and summit station before they are felt at the former. The low-level station should be situated above the irregularities of the earth's surface and away from the artificial conditions existing in cities, so that the normal circulation and conditions of the lower air may be obtained. It might be necessary to have the offices for the receipt and distribution of reports in the cities, while the observing stations should be outside of them, connection being maintained by telephone or messengers.

LOCAL WEATHER FORECASTS AT KINGSTON, R. I.

By Nath'l Helme.

At the request of a representative of the Providence *Daily Journal* I commenced June 1, 1890, sending daily weather forecasts by telegraph for publication in the *Evening Bulletin.* In order to be in time for the first edition of the *Bulletin* which is published at 2 P. M. they are sent from here at 11:30 A. M. for the next 24 hours, and are so published in the paper under the head, "The *Bulletin's* Special Predictions." As regards my plan for forecasting, I take into consideration the local atmospheric conditions, such as the direction of the wind, movement of the clouds, solar or lunar halos, (these last I find are pretty sure fore-runners of a storm), the movement of the barometer, the humidity and temperature, together with a study of the weather maps which I receive daily. I find, and no doubt others do also, that the barometer alone is not always to be

relied upon as a weather indicator, for quite often it falls, at times even rapidly, while the weather continues fair. Then again the indications will be favorable for a storm, the forecast will be made accordingly and will prove a failure, the storm either having spent its force before reaching here, or else changed its course. Local conditions favorable or unfavorable for a storm will sometimes develop which cannot be forecast by the Central Office at Washington, and herein I think is the benefit of having local predictions, as these conditions can better be studied by some one residing in each locality than by one central office. If the country were divided into smaller districts and a Signal Officer placed in charge of each one of the districts as was proposed by the Chief Signal Officer in November, 1889, but which was afterward given up for lack of funds, it might add to the efficiency of the service. My own forecasts have been quite successful I find upon inquiring in Providence, and the verifications have averaged from 80 to 87 per cent. My location is on an elevation 250 feet above sea level, and with one exception is the highest point in this vicinity. The Atlantic Ocean is about eight miles in a direct line south and Narragansett Bay five miles east. The instruments which I now have are self-registering thermometers, hygrometer, barometer and rain-gauge.

LONG RANGE WEATHER PREDICTIONS.

By H. Helm Clayton of the Blue Hill Observatory.

"Long range weather predictions" is a title which has been used to describe predictions of the weather extending one or more days in advance. In the present paper, however, the expression will be used exclusively for such predictions as attempt to describe the general characteristics or most prominent features of the weather to be expected during some future interval of many days, as a week, month, or year. For example, an attempt to tell whether next week will be generally dry or wet, or whether next winter will be very cold or mild, will be what is here meant by long range weather predictions.

Predictions of this kind began far back in these days when man first learned that periods of warmth and cold followed each other at intervals of about 183 days, and were much improved when our semi-savage ancestors set up stones whose shadows

would indicate to them the time when the sun reached it farthest point south and so enabled them to more accurately determine the time when returning warmth would be felt. Much later, when knowledge had greatly increased, the almanac makers were able to give much improved forecasts of this kind by filling in the details of the annual climatic change, as for instance when snows, and storms, showers, and thunder-storms were most to be expected, by writing along a column of dates at the proper place "about this time look out for snow," etc. We are accustomed to smile at such predictions, but a century ago they represented the best knowledge attainable. Since the introduction of instruments, the atmospheric changes have been measured with steadily increasing accuracy and detail, and it has been sought by the method of averages to determine for each place of observation the normal condition of the atmosphere as regards heat, cold, etc., for each month or day of the year.

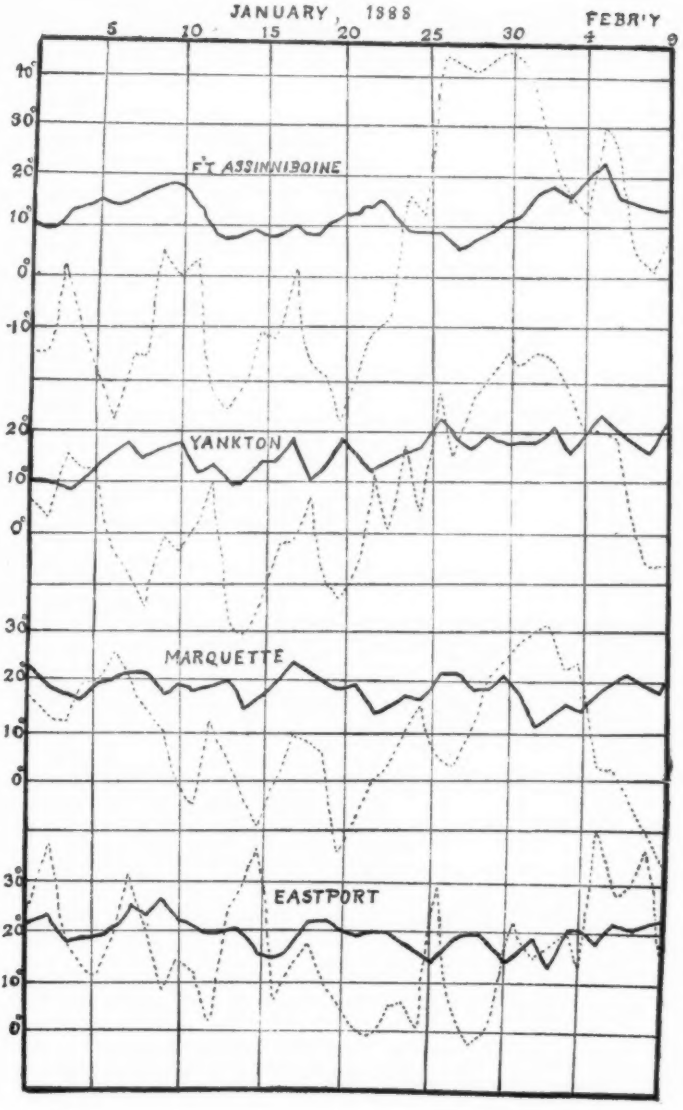
This method during the last 20 years has been brought to a considerable degree of perfection by the organization of weather services all over the world and the stimulus they have given to observations by ships over the various oceans. With this improved data our Hydrographic Office is accustomed to make up forecasts for each succeeding month for the use of mariners, and it is not uncommon to see these published in the daily papers. The data gathered by our various weather services are also very extensively used by the business community for determining in advance the conditions they are liable to encounter, and such use will undoubtedly increase from year to year. But forecasts based solely on such averages cannot hope to reach a high degree of success because the individual phenomena vary so greatly from the average. Thus at Blue Hill Observatory the average temperature of January is 24° F., but it may vary anywhere from 15° higher to 15° lower than this, and the temperature of individual days may vary between 60° F. and —15° F. Again, September is on the average one of the driest months of the year, but during 1888 it was the wettest; while November which is usually the wettest month of the year was last year the driest. The first frost of the winter usually occurs about the first of October, but it may be a month earlier or a month later.

Men have tried to discover the causes of these deviations from the average, and have eagerly sought for other periods differing from, but combined with the annual period. An immense amount of labor has been spent in endeavoring to discover a

periodicity in the weather corresponding to the movements of the moon and although a belief in such a periodicity has gained wide popular credence, the endeavor to prove it by any scientific method has so far been in vain. It has also been endeavored to connect changes in the weather with changes in the spots on the sun, but so far the evidence has been very conflicting. It has moreover been thought that there were weather changes coincident with the rotation of the sun on its axis, but here too the evidence is very meager.

Some men have also believed that they have discovered periodicities in the weather unconnected with any other known natural phenomena. Thus investigators in Europe and America have from time to time announced discoveries of periods in the weather varying from a few days to many years. The only conclusion to be drawn from these investigations however is that if such periodicities do exist there are so many of them of different lengths and varying intensity that their combined action on the atmosphere is to produce a series of very irregular changes without any sign of periodicity on the whole, just as a series of waves of various lengths moving over the surface of a bowl of water and rebounding back and forth might throw the surface into irregular agitation without any visible trace of periodicity. But occasionally a periodicity of a certain length may become so intense as for a while to largely override the other irregular changes and make itself perceptible. For several years during my earliest work in meteorology I labored on this problem of periodicity, and I feel convinced that this last proposition has some basis in truth and I will give some of the evidence from which this is drawn.

It has frequently attracted popular attention that certain days of the week for long intervals continued to be stormy. Thus during last winter rain fell on every Sunday except three out of eleven successive Sundays. During the winter preceeding the tendency to rain on Mondays was equally well marked. If one examines a trace made by a recording barometer during such a period as this there is usually found a more or less well marked tendency to periodic oscillations. One of the best marked examples of this which I have found occurred between May 10 and July 15, 1885, and is decribed in the *American Meteorological Journal* of August, 1885. In this case the periodic oscillations of the barometer were well marked in the central and eastern part of the United States, and for seven or eight weeks


JANUARY, 1888
FEBR'Y
5
10
15
20
25
30
4
9
40°
30°
20°
10°
0°
-10°
FT ASSINNIBOINE
20°
10°
0°
YANKTON
30°
20°
10°
0°
MARQUETTE
30°
20°
10°
0°
EASTPORT

5

periods of cold clear weather alternated with warmer rainy weather with such regularity as to attract frequent comment.

There is also a periodicity of about thirty days in length which occasionally becomes noticeable. Thus during an interval of ten or fifteen days the weather will be wetter and warmer and the pressure lower than the average followed by a period of similar length with less rain, lower temperature and higher pressure. Such oscillations may continue for an interval of several months. I pointed out several cases of this kind in the AMERICAN METEOROLOGICAL JOURNAL during 1885, and in the *Boston Transcript* during February, 1887. This periodicity has since been also noted by one or two other observers. Several workers in Europe have pointed out similar periodicities of still greater length, some of them covering intervals of many years in each oscillation, and at a recent meeting of our Society it was shown that periods of warm and cold winters have followed in New England at intervals of about twenty years.

This, then, is one direction in which we may perhaps look for improved long-range weather predictions in the future.

Another promising direction for research is concerning the apparent slow movement from place to place of abnormal weather conditions. About ten years ago Mr. Chambers gave data to show that periods of drought and famine began in Africa and slowly progressed eastward over India and the adjacent Provinces, occupying many months in the translation.

In November, 1884, Mr. L. A. Sherman stated in the AMERICAN METEOROLOGICAL JOURNAL that he had several times been able to trace areas of abnormal rainfall or areas of abnormal cold which moved slowly eastward, and occupied several weeks in passing from the Rocky Mountains to the Atlantic coast.

In the same JOURNAL for July, 1885, I showed that an area of severe cold during the winter of 1884–5 moved from the Northern Rocky Mountain slope very slowly toward the Atlantic coast, occupying about four months in its progress. During January, 1888, a similar area of abnormal cold moved from the Rocky Mountains to the Atlantic coast as shown by the temperature curves for selected stations given in the *United States Weather Review* for that month. The curves for the four most northern stations are reproduced on the accompanying diagram. Assiniboine, Marquette, and Eastport are nearly on the same latitude. Marquette is about one thousand miles east of Assiniboine, and Eastport is about one thousand miles east of Mar-

quette. Yankton is about half way between Assiniboine, and Marquette but nearly three hundred miles south of a line joining the two. The full curves show the average temperature at each station for a number of years. The dotted curves are drawn from the mean daily temperatures obtained at each station from Jan. 1 to Feb. 10, 1888. On examining these curves it is seen that at each station there was a long period of abnormal cold followed by a period of abnormal warmth and moreover these occur successively later at the more eastern stations.

The prolonged period of abnormal cold began at Assiniboine on Dec. 25, and the greatest departure below the normal occurred on Jan. 6. It began at Yankton on Jan. 5, and the greatest departure occurred on Jan. 14. It began at Marquette on Jan 7, and the greatest departure occurred on Jan. 20. It began at Eastport on January 16, and the greatest departure occurred on Jan. 29. This indicates that the area of cold moved slowly eastward at the rate of about five hundred miles each week. The stations on the same latitude farther south show that this phenomenon extended entirely across the United States

This period of cold was followed by a shorter period of abnormal warmth which occurred about ten days later at Eastport than at Assiniboine, indicating a velocity of something over one thousand miles a week. This is more rapid than the movement of the preceding area of cold but still much slower than the warm waves accompanying our usual winter storms which move with a velocity of nearly one thousand miles a day.

Another promising direction for research on this question has been indicated by the cloud observations at Blue Hill Observatory as was pointed out in *Science*, for March 29, 1888. It has been found that when the highest clouds for long intervals move with unusual velocity, storms also move with unusual velocity, and changes from rain to sunshine and from warmth to cold are unusually frequent, in other words, that the variability of the weather is very much increased.

Moreover it has been found that the rapid movement of the higher cloud can usually be traced to the setting in of colder weather to the north causing the air in that region to be condensed and thus causing the air from the top of the more expanded and hence more elevated air columns in the south to

flow toward this region with unusual rapidity. This air, however, does not move directly from the south but is deflected by the earth's rotation and moves from the SW or W. Thus the appearance of very rapid moving upper clouds from the W or SW may not only give information of much colder weather to the north, but may warn us of the beginning of a period of great variability in the weather, and this knowledge is as important as the knowledge that the average conditions of any period are to be above or below the normal. A period during which days of unusual warmth were followed in rapid succession by days of extreme cold might have an average temperature but little below the normal and yet be attended by much more suffering than a period of uniform cold whose average temperature was much below the normal.

Another promising line of research has been pointed out by Teisserenc De Bort, of the French Meteorological Bureau, who has made a study of the permanent areas of high and low pressure in the atmosphere and their changes. He believes that a knowledge of these changes will throw light on the abnormal weather conditions which at times prevail for long intervals over various parts of the world. Recently he and M. Raymond appear to have made considerable progress toward determining the details of these changes.

There are hence five lines of research which promise to furnish information for making long range weather predictions.

1st. The more accurate determination of the average conditions of each time of year and the probable deviations from the average. This may be called a study of the annual periodicity.

2nd. The study of what may be called irregular periodicities, or apparent periodicities which vary much in intensity from time to time and whose causes are not yet known.

3rd. The slow movement of abnormal weather conditions from place to place, usually from west to east.

4th. The study of the movements of the upper air which are undoubtedly connected with weather conditions prevailing over large sections of the earth's surface, and may hence furnish information of changes taking place outside the field of our observations and also throw light on the causes controlling the variability of the weather.

5th. The study of the permanent areas of high pressures in the atmosphere and their variations.

LOCAL WEATHER FORECASTS AT NEWBURYPORT, MASS.

Mr. Francis V. Pike mentioned some of the difficulties encountered in making the local predictions for Newburyport which are published in the *Herald* of that city, the chief difficulty being that the maps charted in Boston, from which the predictions are made, are not received until from 9 to 13 hours after their issue. Mr. Pike agreed with Mr. Rotch as to the advisability of decentralizing the Signal Service weather predictions.

Prof. W. M. Davis spoke of the need of well-trained and skillful weather predicters and advocated the establishment of a suitable course of instruction in the schools and colleges.

WEATHER PREDICTION IN THE STATES AND ITS IMPROVEMENT.

By M. W. Harrington.

Some months ago the following appeared in the Detroit *Tribune* as, in substance, part of a decision by a judge of the United States district court:

"Proverbially, indications of the weather are unreliable. From the ancient ground hog to the modern superintendent of the weather bureau at Washington, the weatherwise are as often false as true prophets and their miscalculations are daily subjects of good humored derision by the public. Useless as are the monitory storm flags sometimes, not infrequently they are hauled down amidst the laughter of the people, from cloudless skies radiant with sunshine. But there was no cautionary storm flag on the government station nearest to Thunder Bay at Alpena on that morning, and the officials whose business it was to look out for it had not detected this storm at or prior to the time when Captain Bennett was passing Thunder Bay lighthouse."

This was so much like the language of an attorney that, on reading it, I looked back to see if it was not possibly argument and not decision,—but it was certainly given as part of the decision of a judge from the bench. It appears that it was a matter of evidence that there was no cautionary signal displayed at the time and place in question, but that the predictions of our weather service are as often false as true is a gratuitous

assertion, unseemly under the circumstances and untrue in fact. A brief investigation, open to every citizen, would have shown the judge that it was untrue.

The percentages of verification are published regularly in the *Monthly Weather Review*, and also in the "Annual Report of the Chief Signal Officer" where any one who wishes may consult them. Since July, 1887, they have been formed on a plan which has undergone but slight and unimportant variations and have been intrusted to a special officer who took no part in the predictions. The correct estimate of the verification of a weather prediction is a difficult matter; since the date mentioned a process has been employed in the Signal Office like that given in pages 19 to 32 of volume VI. of this JOURNAL. While not absolutely satisfactory from a theoretical point of view, it is a good practical method, and certainly does not err in the direction of favoring the forecasting officials. The percentages which follow are given separately for the weather (that is, the cloudiness and precipitation), for the temperature, and for the wind (force and direction, and tested by the justification of cautionary signals). This is the manner in which they are printed by the Service. The average percentage of verification of the weather prediction is obtained by multiplying the weather-verification by three, each of the others by two, adding and dividing the sum by seven. This is also the Signal Service method. It gives a somewhat greater weight to the weather-verifications, possibly because these are more comprehensive, possibly because they are considered on the whole the more important.

The average verifications for the years selected are as follows:

	Weather.	Temperature.	Wind.	Average.
July, 1887, to June, 1888.....	79	74	76	77
July, 1888, to June, 1889.....	84	76	67	77
July, 1889, to June, 1890.....	85	79	67	78
July, 1890, to January, 1891	86.5	79.5	69	79.5

From this it appears that the weather is predicted with satisfactory approximation five times in six, the temperature more

than three times in four, and that the cautionary signals are justified about two times in three. In general, the predictions are now justified about four times in five, and this is a very different record from that attributed to the Service by the judge already mentioned.

The inequality of the percentages for the different elements of prediction are very noticeable. Under weather, the condition of "fair" is easy to predict and when fair weather has once set in it is likely to continue for several days, or even weeks in autumn. It is not so easy to predict rain or snow and the most of the failures, under this head, are due to failures to forecast rains which actually take place, and this is true notwithstanding that rain may be predicted for a wide territory, and for justification it need not fall over the entire region.

Forecasts of change of temperature are more precise and verification is not so great. The prediction of strong winds is still more difficult, and the prediction must include approximate time, place, direction and velocity. The prediction is not completely justified unless all these particulars are foretold within a certain narrow range fixed by general rules. It is a misfortune that these predictions cannot be made with more exactness. A justification of two in three is hardly enough to vindicate an expectation that they will be useful to commerce. It may be that an improvement might be introduced by limiting the display of cautionary signals to times when it is not a fresh breeze but an actually dangerous wind which is approaching. It is a common remark among masters on the Great Lakes that a Signal Service cautionary signal is more likely to mean a highly favorable wind for sailing than a dangerous one, and those who guide their sailings by it often find themselves outdone by rivals who pay no attention to the signals. In ports, as at Chicago, the masters often call on the signal officer and learn the details about the approaching wind, and the Service thus does a great deal of good work without getting credit for it. Possibly by a limitation of the predictions to those winds which promise to be really dangerous, the Service could get a higher percentage of verification and also could give greater satisfaction to the public.

The small table already given suggests also a gradual improvement in the verifications. As these are estimated on a uniform rule this must mean an encouraging improvement in the predictions themselves. Each unit's gain in the percentage is a

matter of importance and interest. The gain of one per cent. in a year is an achievement of considerable significance. The method now employed in making estimates of verification is a rigid one and is not over-favorable to the predicting officer; any increase in percentage will have to be won in spite of it, and a gain of one per cent. will grow more and more difficult as the average creeps up, if it really continues to do so, toward 100. So important is improvement in this direction that it will be of interest to examine the percentages for different officers, for the different seasons and for different quarters of the country.

As to individual forecasts,—the work is done by a small but variable number of officers. The entire number employed in it since the work began, now nearly twenty years ago, would probably not surpass a score. Major Dunwoody seems to have had the most practice. General Greely was, years ago, the predicting officer for a few months, and obtained a high percentage of verifications. The forecasting officers are usually from the army, though a few of the civilian officers have been employed at it. Each officer takes this work for a single month. It is very responsible work; it is also very exacting. The predicting officer must work for a short time under high pressure, and the work comes late at night. While employed on this duty he is relieved from all other. To test what part personality plays in the justification of predictions, I have taken the monthly percentages from July, 1888, to January, 1891, and averaged them for each officer. I have rejected cases where the service of an officer was less than three months, as not likely to give a good average, and I give the number of months employed in getting the mean for each.

	Months.	Weather.	Temperature.	Winds.	Average.
Major Dunwoody	7	86	82	71	81
Captain Allen	6	85	79	71	79
Lieutenant Glassford	3	85.5	79	69	79
Professor Hazen	7	87	76	66	78
Lieutenant Thompson	5	83	74	60	74

The last column is made from the three preceding in the way already mentioned. It appears that the variations between

individual forecasters is large enough to be taken into account. The successful prediction of the weather is, of course, for the most part the result of information and practice, but it seems to be also in part due to a peculiar sagacity which it is impossible to define. It is, like sagacity in other pursuits, a matter of mental make-up which can not be told beforehand, nor supplied when non-existent. It certainly is not a matter of general intellectual ability nor of great learning, for one of the most able and learned men in the Service is also one of the less successful forecasters. A policy which the Signal Service seems to have adopted of late years, and certainly a very prudent one to adopt, is that of apprenticing predicting officers on the Pacific coast or elsewhere, where the predictions are more local and apparently easier to make.

To show the relation of verifications to the seasons, I have drawn up the following table. It gives the means for the period from July, 1887, to January, 1891.

	Weather.	Temperature.	Wind.	Average.
Winter	83	80	74	80
Spring	83	73	72	78
Summer	82	76	63	75
Autumn	85	77	70	78

In general, the predictions are worst in summer and best in winter. The progress of storms over the states is most regular in winter, most capricious in summer. The prediction of weather is best in autumn,—the season of settled "fair" weather. The prediction of winds is especially poor in summer and exceptionally good in winter,—the reverse of its usefulness in the northern, and more commercial, states.

The distribution of verifications over the states shows some curious and important features. To test it I have taken the two years from July, 1887, to June, 1889. The later years have not yet been printed in a form available for such use. The figures are from those of the Signal Service and seem to be what I have called the "average" verification, though I do not find them defined. At least they will show the comparative verifica-

tions. Grouping them in suggestive divisions of the country, I find:

For the North Atlantic Coast to the Potomac	78
For the South Atlantic States	81
For the Gulf Coast States	81
For the Northern Boundary, New York to Dakota	77
For the Pacific Coast States	84
For the Interior States east of the Mississippi River	79
For the Interior States west of the Mississippi River	78

It appears that there is an appreciable difference in the verifications for different parts of the country. The Northern Boundary States afford the worst verifications,—and this includes the Great Lakes. Pacific Coast States, curiously enough—the boundary over which the great storms make their advent into our territory—have the highest verification. Figures given for these states are only for the last of the two years but to those who have followed the Pacific predictions from the establishment of a prediction office at San Francisco, it has been evident that the forecaster is unusually successful there. An evidence of his success appears in the popular content shown in the tone of the newspapers of the western coast.

Next in efficiency to the Pacific States come the Gulf States and then those of Southern States on the Atlantic border. In general the predictions are better for the Southern States than for the Northern. The Interior States east of the Mississippi River come next, and then the Western Interior States and the Northern States on the Atlantic border.

It is a misfortune for the Service that the Eastern States of the North, and those of the northern boundary, should be precisely those giving the poorest verifications; for this covers the most thickly populated region and the region most devoted to commerce. The reason may lie partially in the fact that the stations are here nearer together and the tests of verification more complete, but it is probably due rather to the fact that this is the region of most unsettled weather and is, moreover, the region exposed to invasions from the arctic continental area to the north of us. The Service should make the most of its success in the Southern and Pacific States. This appears unknown to the critics of the press of the northeastern section. A more complete exchange with Canada would also give, probably, better warning of weather advancing from higher latitudes.

But our enterprising weather service has not contented itself with predicting the weather a single day in advance; it has for

some time tested the success of predictions for the second day in advance, and sometimes even for the third. This has, however, been done under conditions quite different from those used for predictions for the next day. The former have been left to the option of the forecasting officer. His regular predictions must be made whether the indications of his map are positive or not; the predictions for two or three days may be made when he thinks that the indications justify them. The latter is the easier task; peremptory predictions two or three days in advance would, of course, be often very unsatisfactory.

Beginning with July, 1889, I find eighteen months with verifications given for the second day in advance, — every month up to January, 1891, except August, 1889. The weather and temperature predictions were made but not the wind, and, with one or two exceptions, they appear to have been made by the regular predicting officer. The mean of the verifications of weather for two days in advance for these months is 78 and the individual months ranged from 57 to 100. That for temperature is 81, and the individual percentages run from 65 to 95. Evidently, under these circumstances, the predictions for weather are almost as successful as for one day in advance, while those for temperature are even more successful.

Of predictions for the third day in advance I find records for only eight months. The mean for weather is 93, running from 75 to 100, and that for temperature 60, running from 14 to 100. The percentages for weather are surprisingly uniform, those for temperature quite as surprisingly the opposite.

Some forecasters are much more daring than others in using the option of predicting for two or three days ahead. We may interpret the results to mean that when due caution is used such predictions may be serviceable, and it will occasionally happen that they may be made for a still longer time in advance. Failure in such long-time predictions is not so injurious to those who trust them, as they can be corrected in subsequent reports if they prove erroneous; they would be useful and would attract popular attention if successful. They also perform the service of making the forecaster examine more closely the weather-conditions on the map before him, and thus render more exact his predictions for a single day ahead.

The Signal Service has also endeavored to accomplish the prediction of the advent of cold-waves, but the official verifica-

tions give the officer to whom these predictions are intrusted a verification of not much above 50 per cent. This is a source of surprise to meteorologists in the Northwest, for we have had the impression, derived from following the signals at our individual stations, that the cold-wave predictions compared favorably with the others. Had I been asked to guess at the verification of cold-wave signals, I would have put it at 75 per cent. at least. It is quite possible that the rules of verification, or the official definition of the phenomenon are, unintentionally of course, more exacting for this officer than for others, and perhaps more exacting than the public demands. Certainly the popular estimate in the Northwest—the territory most involved in cold-waves—is as high for these predictions as for the others, higher than for those represented by cautionary signals.

As to improvement in weather predictions several suggestions occur at once, and some of these have been already made. The *personnel* of the forecasting board is a matter for special solicitude. Preparatory practice should always precede appointment to the general board, and this can be found in the local forecasting stations. The board should be so small as to keep the members in constant practice, yet so large that death or resignation would not sensibly impair its efficiency.

The weakest spot territorially in our predictions appears to be in the states along the northern boundary east of the Rocky Mountains, and, for more reasons than one, this region should receive special care. The Signal Service formerly had a special predicting officer at St. Paul but he seems to have been withdrawn, though I do not know for what reasons. This appears an eminently prudent arrangement and the officer stationed there should have the use of Canadian observations as far north and west as possible. The northern boundary, about and west of the Great Lakes, is a critical line across which the weather conditions frequently pass. The Canadian service now coöperates with that of the United States and can be relied on to continue to do this, and this coöperation should be made as complete as possible, especially for the region east of the Rocky Mountains. It is an open question if St. Paul is the best station for this predicting officer. The weather changes at Mackinac island used to be significant of those which we would have in lower Michigan. Sault Ste. Marie and Marquette might be better than St. Paul, or even Duluth might be an improvement.

Some improvement in predictions might possibly be made if the upper cloud movements were observed and reported more exactly. Professor Abbe has made a report on various features aiding the general problem of deductive methods of weather prediction, and the report has recently been printed. (See "Annual Report," 1889, 2d Part). This report is full of suggestions, both for the forecaster and for the ordinary meteorologist. Its methods are rather tentative than positive and the author does not seem to sufficiently distinguish between the occasional great storms which persist for many days and travel with fair uniformity on beaten paths, and the more local and more capricious, though not necessarily smaller, storms, where topography plays an important part. The discrimination between them, however, is an essential part of the tact of the forecaster, and their treatment by him must be quite different. Various of the tests of future progress which are suggested by Professor Abbe might be successfully applied in the latter and would form a very useful resource for the forecaster, while they would fail for the former. In getting secondary or local storm probabilities many of Professor Abbe's suggesstions will probably prove of decided usefulness.

The improvement in weather predictions which lies nearest at hand consists, undoubtedly, in the further adoption of the principle of local centers for prediction. The results for the Pacific Coast have been very happy and experience has abundantly shown that a local predicter near Boston can, with the aid of the Signal Service weather maps, make predictions for that city which surpass in justification those made from Washington. This must necessarily be the case, other things being equal, if the local forcaster has at his disposal all the information available for the general forecaster, for the former has in addition a narrower field to predict for and has the advantage of any local signs which are not involved in the observations at the various stations.

How thickly the local forecasting offices should be scattered is a detail to be decided partly by questions of expense and executive requirements, partly by the results of actual trial. It appears now that the Pacific Coast States may properly get their indications from one office. Perhaps a single office would suffice for the northern boundary from Lake Huron westward. On the other hand, there would be advantages in having a local

predicting center for each large state and for such groups of small states as New England.

An improvement in the usefulness of the predictions to the individual, who is interested in them and has the necessary appliances, is easily accomplished. The ray of light which reaches him from any quarter of the heavens betrays, when properly questioned, much information concerning the part of the atmosphere which it has traversed. The rainband spectroscope interprets something of this, the telescope something (see Ventosa, this JOURNAL, VII. 89), and something is shown by the twinkling of the stars, especially when questioned by Montigny's simple mechanism. The upper clouds also give much information, and if the individual in question is well posted on the theory of storms and their motions and has in hand the report of the nearest predicting officer, he can employ the most commonplace weather signs to give precision to the official forecast as applicable to his own locality.

But these suggestions are all of minor importance as compared with that of perfecting the theory of storms. It is of comparatively little consequence to pay attention, empirically, to individual symptoms of atmospheric disturbance; it is the general pathology which must be understood before much real and positive advance can be made in weather prediction. This advance will depend largely on individual meteorologists who study scientifically the data collected by the weather services. It is one of the chief functions of these services to make their observations available to such students—and they are quite as likely to be private students as members of the services. It is also one of the functions of the weather services to furnish such students with all classes of observations which the latter cannot make for themselves. In the present outlook of meteorology it appears that what we now most need are observations taken as high up in the atmosphere as possible. Their interpretation has proved very instructive in the last few years. The Signal Service has appreciated their value and has made a goodly number of them. They should be continued and the plan expanded,—so far as is permissible under the statutory limitations of the new Weather Bureau.

It is proper to add that it is a vain expectation that all weather phenomena can at all times be predicted with any very great approach to precision. The weather is a multiple periodic phenomenon and is much modified by details of topography.

What advance may be reasonably expected in predictions may be estimated by observing the state of prediction for the analogous phenomenon of the tides. The theory of the tides is simple and complete, the analysis which has been applied to it is rigorous and detailed, the periodic phenomena involved in it are known, yet it is possible to predict a tide at any port only by long-continued observations at that port. Topography here plays the part of a source of perturbations, local in character, yet so complex that forecasting on general principles is not practicable. The weather is due to complicated actions and reactions, occurs in an elastic medium, and is much more subject to perturbations. We can not hope to predict it more successfully than we do the tides. This should be understood by the public for it is much easier to satisfy reasonable expectations than unreasonable ones.

THE ZODIACAL LIGHT AS RELATED TO THE AURORA.

By Orray Taft Sherman.

The aurora is generally regarded as an electric discharge in the upper atmosphere. To supply the electricity necessary purely terrestrial forces are involved, yet the best determined periods of auroral variation follow cosmical periods. To account for these from terrestrial causes is difficult. One may suggest that they are secondary effects of other primary effects of cosmical action. But the suggestion must stand upon its own basis, and the amount of probability depend upon the greater or less cogency of the reason presented. As far as terrestrial causes are intermediate it is to-day, we think, a naked suggestion. Much light may be shed on the subject by the comparison of the curves presented herewith.

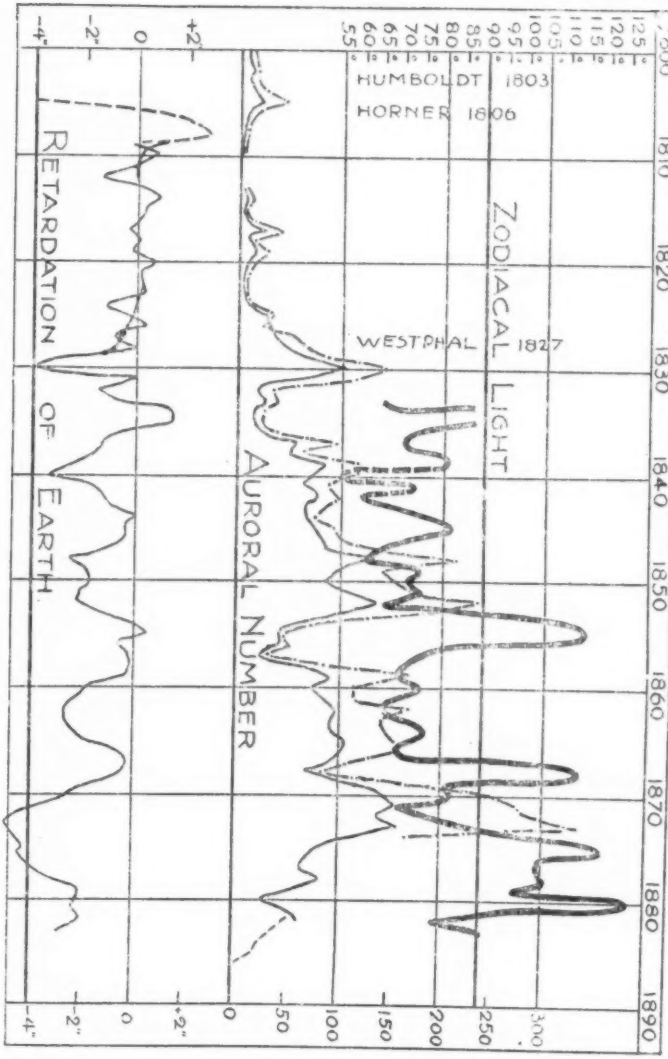
The first column of the following table under the head "Relative Elongation of the Zodiacal Light," presents the summation of many hundred (1750) observations—all that a very extended search has revealed reduced to what they would appear if seen in March. March was chosen as being the month in which observations were most numerous. It happens that the elongation then is much less than in the four months immediately preceding. It would follow then that what here seems far removed from the earth would yet meet it at certain portions of the year. The method of deriving the curve has already been given in "*Himmel und Erde*," (July, 1889). The values here given differ from

those given there, first, because they represent an increased number of observations, and secondly, because here no attempt has been made to allow for a scant number of observations by smoothing the curve. Indeed the irregularities seem to follow the same rule as the larger variations.

In the second column is given, under the head "Corrections to the Earth's Calculated Longitude," that part of the amount by which the observed position varied from the calculated, which is probably due to the Zodiacal Light. It summarizes over thirty-six thousand observations. The complete account is given in Dr. Gould's *Astronomical Journal* (No. 232).

In the third column is given Fritz's Auroral numbers for Europe south of the polar circle, and in the fourth column his relative numbers for Europe. This series, lying as it were on the edge of the Auroral belt, has seemed best suited for our purpose. The other series present a similar result, but perhaps not so clearly.

Year.	Relative Elongation of Zodiacal Light.	Observed Correction to Earth's Calculated Longitude.	Aurora for Europe So'th of Polar Circle.	Auroral Relative number.
1801	Observed by	.....	7	12
1802	Humboldt in	.....	11	16
1803	1803	.....	8	15
1804	.....	.....	12	23
1805	.....	+4".04*	22	44
1806	Horner--1886	—0.21*	8	13
1807	.....	+2.24*	6	8
1808	.....	+2.84*	2	3
1809	.....	—0.20*	1	1
1810	.....	+0.80	1	2
1811	.....	—0.11	0	0
1812	.....	—1.47	0	0
1813	.....	+0.27	2	4
1814	.....	+0.93	6	12
1815	.....	+0.30	2	4
1816	.....	+0.01	4	6
1817	.....	—0.39	26	40
1818	.....	+0.05	8	13
1819	.....	—0.21	15	27
1820	.....	+0.70	8	13
1821	.....	+0.18	5	8
1822	.....	+0.20	3	5
1823	.....	+0.41	1	1
1824	.....	—1.12	8	9
1825	.....	—0.40	26	30
1826	Westphal--1826	+0.40	29	25
1827	.....	—0.93	37	53
1828	.....	—0.05	47	65
1829	.....	—2.97	62	86
1830	.....	—3.96	103	141
1831	.....	—0.06	69	112


HUMBOLDT 1803
HORNER 1806
WESTPHAL 1827
ZODIACAL LIGHT
AURORAL NUMBER
RETARDATION OF EARTH
1800
1810
1820
1830
1840
1850
1860
1870
1880
1890
125°
120°
115°
110°
105°
100°
95°
90°
85°
80°
75°
70°
65°
60°
55°
0
50
100
150
200
250
300
+2"
0
-2"
-4"

Year.	Relative Elongation of Zodiacal Light.	Observed Correction to Earth's Calculated Longitude.	Aurora for Europe So'th of Polar Circle.	Auroral Relative number.
1832	.....	—1.45	20	31
1833	.....	—0.73	27	37
1834	69°.0	+1.42	14	24
1835	87.1	+1.45	15	21
1836	.....	—0.54	23	37
1837	69.0	—1.26	56	95
1838	79.7	—1.46	50	60
1839	83.1	—2.23	85	115
1840	54.9	—3.30	76	117
1841	73.6	—1.66	63	87
1842	60.0	—1.24	79	97
1843	66.3	—0.96	68	85
1844	77.0	+0.03	66	77
1845	82.3	—0.39	76	94
1846	71.7	—0.46	89	99
1847	64.5	—0.87	87	129
1848	60.4	—2.47	129	210
1849	73.9	—1.87	100	140
1850	70.7	—1.73	89	151
1851	74.2	—2.09	101	174
1852	65.4	—2.24	138	233
1853	82.9	—1.59	116	180
1854	108.9	—0.25	59	70
1855	114.8	+0.59	40	45
1856	79.9	—0.61	43	50
1857	75.0	—0.22	20	22
1858	68.9	—0.24	66	86
1859	68.7	—0.28	91	168
1860	74.4	—1.69	78	117
1861	66.2	—2.25	83	117
1862	66.2	—2.55	99	165
1863	69.9	—2.66	96	143
1864	75.4	—2.42	93	144
1865	70.7	—2.05	109	155
1866	68.6	—0.86	105	159
1867	72.0	—0.24	90	117
1868	112.8	—0.37	70	84
1869	80.4	—1.00	113	168
1870	81.7	—2.33	143	244
1871	69.4	—3.03	159	275
1872	75.8	—4.43	140	282
1873	86.5	—4.87	158	332
1874	101.5	—4.71	101	169
1875	119.0	—4.21	87	.....
1876	103.3	—4.36	72	.....
1877	103.1	—3.92	69	.....
1878	104.9	—3.37	85	.....
1879	97.3	—2.00	67	.....
1880	125.6	—2.08	29	.....
1881	92.4	—2.17	53	.....
1882	78.3	—2.02	64	.....
1883	90.6	—2.80	38*	.....
1884	Probably continued beyond the earth's orbit until 1890.	.....	24*	.....
1885		.....	23*	.....
1886		.....	1*	.....

A moment's inspection of the table shows that from 1806–1827 we have no observation of the Zodiacal Light, slight and irregular variation of the earth's motion, and slight and irregular auroras. For the next fifty years each period of elongation of the Zodiacal Light corresponds with a maximum acceleration in the earth's motion and a minimum in the aurora. Further, it is evident that at the time when the Zodiacal Light is constantly beyond the earth's orbit (1873) the auroras are scant and diminishing. From data in hand, but not complete, it is evident that this relation was maintained for some years further. Thus producing the second long period in the variation of the aurora.

To pass from synchronism to cause and effect is no very difficult matter when we remember what these appearances are. The elongation curve shows us that at certain periods the Zodiacal Light becomes sufficiently elongated to extend beyond the earth's orbit. Observation shows us that it lies about the ecliptic. Its continuous spectrum shows us that it is in part composed of solid matter. Our earth curves show us that at this time it is felt by the earth. We know, however, by observation that the Zodiacal Light spectrum is, especially at the time of maximum elongation, accompanied by the bright line of the aurora. We know, also, that at this time it behaves very like an aurora. It follows that the contact of which we have just spoken is accompanied by an electrical discharge in the upper air. Our auroral curve shows us that a part of the charge which would go to produce the aurora is satisfied. The minimum of the aurora follows.

It is a necessary consequence that the particle forming the Zodiacal Light has an electric charge of the same nature as that of the lower atmosphere. This would also seem to be the meaning of the simultaneous increase in the number of auroræ and the elongation of the Zodiacal Light—a part of the charge of the upper air layers resulting from induction. We are thus led to the same constitution for the Zodiacal Light as others have found for the solar corona, viz: an electrically charged fog of somewhat widely separated particles.

The Zodiacal Light does not, however, lie equally distributed about the sun, but is farthest from the earth in July, and extends beyond it in December and January. Roughly speaking, it intersects the path in October or November, and February or March. The principles that we have just disclosed would,

therefore, lead us to expect a minimum of auroræ in July and December–January, and a maximum in October–November and February–March. The mean of a very large number (15.873) of observations places the maxima at March 18 and November 10, the minima at June 24 and January 4; an accordance as close as might be expected. The consequences which happen from the motion of the earth about the Zodiacal Light are strictly analagous to those which happen when the earth is considered as standing still and the Zodiacal Light is in motion.

It would seem that we may consider the Zodiacal Light as determining the three periods of the aurora. To grant such an influence on the aurora goes a long way to acknowledging its influence upon other meteorological and magnetic phenomena. These we hope to trace later.

FARWELL'S RAINFALL SCHEME.

About twenty years ago Mr. Powers wrote a book on the relation of battles to rainfall but the edition was burned in the great fire at Chicago. A new edition was published in 1890, and the reading of this proved so convincing to Senator Farwell that he carried through Congress, at its last session, a bill appropriating a small sum to test the possibility of the artificial production of rain by means of explosions. The experiments are soon to be tried at some point in the west, presumably on the plains.

The carrying out of the experiments was intrusted to the Agricultural Department. The officials of this department appear, very naturally, to have little confidence in a successful issue to them. Mr. Fernow, Chief of the Division of Forestry, has reported on it to Secretary Rusk. We quote below such facts and statements as seem to be of general interest. Mr. Fernow says:

"By an amendment in the Senate the appropriations for this Division were increased by the sum of $2,000 and the words 'for experiments in the production of rain' were added to the reading of work to be performed under the appropriations.

"At first sight, the reference to this Division of such experiments would appear to have been made by reason of the claimed influence of forest areas upon the distribution of rainfall. It was, however, learned that these experiments were not intended to have such a connection nor were they to be devised for the

purpose of finding out any special means for the production of rainfall, but they were to be carried on upon the assumption that explosions would have the desired effect and the money was appropriated to be expended in the purchase of explosives and in their discharge.

"With such a program the reference was, to be sure, unfortunate, for aside from the fact that neither the Division nor the Department in any of its branches commands the means or the men to conduct such explosions or the instruments which should at least be observed during the explosions in order to arrive at an understanding of the results, should any be attained, the amount appropriated in the absence of such means and persons is so totally out of proportion to the needs of the experiment, and, indeed, to the expected overawing result of controlling nature's most potent and hidden forces, that an attempt to use it in the proposed manner could hardly fail to be barren of results.

"On the other hand, the War Department commands cannon, explosives, and men trained and accustomed to handle them, and in its Signal Service, instruments for meteorological observations and observers, and as long as the experiments are to be carried on upon the assumption that explosives will be effective, I have submitted the propriety of asking the coöperation of the War Department in this matter. I have also submitted, as my opinion, that the assumption for such experiments is, to say the least, hazardous, and that a much better use of the money could be made and valuable results much more likely attained, by devoting it to a series of experiments, which would bring us first nearer to a conception of what forces are at work in producing rain and teach more about the chances of substituting feeble human efforts for grand cosmical causes.

"The theories in regard to the causes of storms, and especially their local and temporal distribution, are still incomplete and unsatisfactory. It can by no means be claimed that we know all the causes, much less their precise action in precipitating moisture. It would, therefore, be presumptuous to deny any possible effects of explosions; but so far as we now understand the forces and methods of nature in precipitating rain, there seems to be no reasonable ground for the expectation that they will be effective. Hence, while I do not believe, contrary opinions of high authorities notwithstanding, that such experiments are necessarily devoid of merit, as long as they are con-

ducted upon a careful, scientific plan and a large enough scale, it would be unreasonable and contrary to the spirit of our advanced civilization to rush into a trial which does not seem warranted by our present knowledge, instead of starting with a series of carefully devised experiments, the first object of which would be to learn something of the effects of explosions upon the atmosphere, a knowledge which we do not possess and which, if not leading to the power of controlling rainfall, may considerably advance our knowledge of meteorological forces.

"It sounds quite simple to try whether explosions will produce the precipitation, but when it comes to practically arranging the trial, such questions as the following it seems must be settled first: What kind of explosive shall be used? Is it intensity or frequency of explosions that should be tried? What amount should be used? What means of exploding are best adapted to the purpose and in what manner should they be employed; how high above ground would the explosions be effective? Lastly, how shall we know whether precipitation was due to the explosions? How far did other conditions influence precipitation, etc.? These are questions which it would puzzle experts to answer on any basis of present knowledge.

"A large number of trials, with all sorts of differences in the conditions, might possibly settle some questions, but, unless careful observations, not easily devised, were made simultaneously as to the effects upon primary conditions, under which the result is obtained, our trials would lead us no further than we are at present, namely, to the very unsatisfactory assumption upon which we based our trials.

"Under these circumstances, up to the present writing no attempt has been made to advance this problem.

"Meanwhile I have tried to trace the history and scan the evidence which has led to the assumption that explosions will produce precipitation, and incidentally I have also inquired into other means of artificial production of rain which have been proposed.

"In recent times two artificial causes of rainfall have exercised the minds of speculative meteorologists, fire and explosions.

"It is a current belief that large fires and the cannonading during battles cause precipitation.

"Singularly enough the belief that battles occasion rain is older than the invention of gunpowder. Thus we read in Plutarch: 'It is a matter of current observation that extraordi-

nary rains pretty generally fall after great battles;' and he explains it, that either some divine power in this way cleanses the polluted earth or that moist and heavy vapors steam forth from the blood and thicken the air, and make the moisture fall.

"It should also be borne in mind by those who believe in the effectiveness of cannonades in bringing on storms, that according to Arago ('Thunderstorms,' pages 164–165) during the latter part of the last century, and as late as 1810, it was a popular practice in the communities of Southern France to fire off batteries, especially kept for the purpose, in order to *dispel* violent rain and hail storms, which were undesirable visitors of the region. Arago traced the history of this belief to a naval officer in that region, who had propagated the practice of navigators of that time of dispelling waterspouts and thunder clouds by that means. Before this innovation the effect was sought by the ringing of church bells. Arago tried to disprove such an effect and to prove the opposite by showing that during the artillery practice at Vincennes, out of 662 days each preceding, following and during the practice, there were cloudy 128, 146 and 158 days respectively. This seems to be a rather small percentage to establish the positive effect he claimed; however, it may prove the futility of the opposite belief.

"The only actual experiment that has come to the writer's notice in which a cannonade seems to have been directly effective was reported a short time ago in *La Nature*, and is vouched for by a M. Guillaume. A French artillery division moved out for a sham battle; when ready for action a dense mist arose, which obscured the entire valley so that one could not see 300 feet. One of the officers, recalling the asserted influence of cannonades, proposed to try the remedy; four mortars fired 1-pound charges, first eight shots in succession, then two salvos of four each, when suddenly the mist disappeared, clearing the valley for three miles, and a fine, drizzling rain fell, which, as the cannonading of the sham battle continued, did not cease all day, sometimes falling in heavy showers. I have not been able to ascertain the authenticity of this report and the general weather conditions prevailing at the place and time.

"Our present meteorological knowledge does not give much hope for success by this method of rain production. A method which appeared more reasonable, or at least one that seemed to be in agreement with our present theories of storm formation, was proposed by the author of these very theories.

"The belief in fires and rains as cause and effect is also a very old one, but it was Espy who first, in 1839 (having shown that a column of air rising to a height where, owing to diminished pressure, it would expand, was by this expansion cooled, thereby condensing and eventually precipitating its vapor), proposed experiments 'to see whether rain may be produced in time of drought, making a large body of air ascend in column by heating it.'

"Besides his general theories, which were accepted as most reasonable explanations of the formation of storms, he brought forward evidence to show that volcanic eruptions and large fires (he also refers to the cannonade of battles, evidently, as producing heat) were followed by rainfall.

"The evidence is of the same kind as that brought to show the effect of cannonades. The negative cases, where conflagrations failed to produce rain, would probably be found as numerous as the positive ones. In almost all those which allowed an analysis of atmospheric conditions, these were favorable to cloud formation, namely, a high dew-point and a calm and sultry air, which Espy admitted were needful conditions, and which, at least the former, are rarely present in times of drought. The great fires of London and Chicago are cases in point. In forty-two large fires and two serious explosions occurring in Australia during twenty-one years, 'there was not one instance in which rain has followed within forty-eight hours as an evident consequence of the fire.'

"It will again be interesting here to note that Volta the great physician, proposed to use fire for the very opposite effect, namely, to dispel thunder clouds.

"The impractibility of this method was exposed by Mr. H. C. Russell, government astronomer at New South Wales, who showed that in order to increase by 60 per cent. the rainfall at Sydney, where the average humidity is 73, and wind velocity 11 miles per hour, at least 9,000,000 tons of coal would have to be burnt daily, since it would be necessary to raise a column of air over a surface of at least 10 miles by 1,000 feet to a height of 1,800 feet; and while there may be found some flaws in his calculation, it gives an approximate idea of what forces are to be dealt with and of their enormity.

"Mr. Russell, who was then (in 1884) antagonizing the idea of inducing the Australian Government to engage in experiments like those now proposed here, concludes:

'It would seem unreasonable to hope for the economical production of rain under ordinary circumstances, and our only chance would be to take advantage of a time when the atmosphere is in the condition called unstable equilibrium or when a cold current overlies a warm one. If, under these conditions, we could set the warm current moving upwards and once flowing into the cold one, a considerable quantity of rain might fall; but this favorable condition seldom exists in nature.'

"Professor Henry, one of our most enlightened and unprejudiced physicists, expresses himself as follows in regard to Espy's propositions:

'I have great respect for Mr. Espy's scientific character, notwithstanding his aberration in a practical point of view as to the economical production of rain. The fact has been abundantly proved by observation that a large fire sometimes produces* an overturn in the unstable equilibrium of the atmosphere and gives rise to the beginning of violent storms.'

* * * * * * * *

"Altogether, the theories for storm formation, while perhaps sufficient to explain the general philosophy, do not seem capable of explaining satisfactorily the smaller modifications and side shows, as we may call the exhibition of local showers, thunderstorms and squalls. Nor can it be said that the detail of the manner in which the vapor condenses and the rain drop is formed, or in fact the forces active or conditions necessary in this condensation are fully known or understood. Who could, for instance, account for the fact that the dew-point may be at and above 100 without precipitation occurring? We know some seemingly necessary conditions, but we do not know all. For want of experimental knowledge meteorology seems to have lagged behind the times.

"While the mass movements that are calculated to satisfy the existing theories of general storm formation may be necessary for such formation, is it altogether inconceivable or unphilosophical to think that other molecular forces may participate and, in fact, be a condition *sine qua non* in forming precipitation? Is it not also conceivable that, as in many chemical reactions, it is only necessary to give the impetus to molecular motion to initiate the change, metabolism, which being induced at some center of formation, spreads and assumes greater and greater proportions, similar processes may take place in the condensation of vapor from the air? If such were the case the expectation of at least a partial control by human agency might well be realized. Suggestions of this kind have been made

*Should perhaps read, "is accompanied by."

before, not only by those who would suggest any forces to explain phenomena, without understanding the possibilities of such forces to do the work, but by physicists upon experimental basis.

"Laboratory experiments by Mr. Aiton seem to indicate the presence of dust particles as an essential condition for rain production; and, although Professor Abbe "dismisses from consideration at present" the influence of atmospheric electricity in storm production, he does so only because we know too little about it, and because an assumption of such influence does not seem to help the accepted theories of air movements as sole causes. Even so, he is compelled to admit that "actual measurements of electrical potential would seem to show that two masses of air in extreme conditions may attract or repel each other electrically to an extent sufficient to produce appreciable phenomena of motion even in comparison with the far more important motions produced by solar heat and terrestrial gravity."

* * * * * * * *

"In conclusion I may refer to the observation that dust particles are found always charged with positive electricity, which may account for their office in rain production, and that experiments by Professor Trowbridge, of Harvard, on the effect of flames upon the electric conditions of the air would lend countenance to the belief in the effect of fires on rainfall, while the possible origination of electric currents as a result of friction in cannonades is suggested by Mr. Powers as an explanation of their assumed effect.

"We may say, then, that at this stage of meteorological knowledge we are not justified in expecting any results from trials as proposed for the production of artificial rainfall, and that it were better to increase this knowledge first by simple laboratory investigations and experiments preliminary to experiments on a larger scale.

"If explosions are to be tried at once then it would be necessary at least to take all possible precautions to ascertain the state of the atmosphere in all particulars before, during, and after the explosions, and to conduct and refer to the experiments rather as investigations into the effect of explosions upon the atmosphere than with the ultimate desired result in the foreground."

We have but two additions to make to what is said by Mr.

Fernow. The first is that gross experiments by the explosion of gunpowder in the air are not of the proper sort for getting fundamental and useful knowledge as to the effects of explosions or smoke on condensation. They do not permit the control of the details, such as temperature and humidity, present before and after the explosion, nor the determination of the quantitative effects, if any. Such experiments are coarse, expensive, ineffective and ambiguous, and to make them now is to begin at the wrong end of the investigation.

The second addition to Mr. Fernow's remarks is that we can not make rain out of dry air. Moisture must already exist in sufficient quantities if rain is to be made. Now the moisture does not generally exist in sufficient quantities over the Plains. Admitting that explosions do determine rainfall in some cases, and this may be admitted for the sake of argument though it is by no means proven, yet they can make rain only when the moisture is present in sufficient quantity, When this is the case rain is likely to fall without the explosions. In short, when it is likely to rain anyhow, explosions may perhaps precipitate the rain; when it is too dry to rain, no quantity of gunpowder or dynamite can squeeze out of the air water which is not already there.

CORRESPONDENCE.

PROGRESS OF METEOROLOGY IN 1889.

To THE EDITORS: The review in the April number of this Journal of Mr. G. Curtis' paper on the progress of meteorology, published in the Smithsonian Report for 1889, furnishes an interesting instance of how differently two persons may view the same thing. I was so much impressed with the greater notice in this report of American meteorological work, as compared with previous reports, that I was led to speak of it more than once; yet the review of the report in THE JOURNAL especially criticises it for the lack of such notice. It is true that the meteorological work of neither Europe nor America received that fullness of report which might seem desirable, but the writer was confined to eighty pages, and hence it was necessary that he should confine his notice to the work of the year which seemed most important. That different persons would have chosen different things is almost certain; but Mr. Curtis is to be com-

mended for the great labor and care devoted to the work, and to the clearness and accuracy of description in what seems to the writer an excellent resumé of the progress of meteorology during the year. The reason some of the books written during the year were not given in the bibliographical list was, probably, that they had not been received up to the time the list was printed. H. H. CLAYTON.

CURRENT NOTES.

COMPARATIVE OBSERVATIONS WITH DIFFERENT RAIN-GAUGES.—Dr. Hellmann publishes in No. 3 of the *Abhandlungen des K. Preuss Met. Inst.* an account of his observations of this sort. The instruments used were two of Von Bezold's form, six of Hellmann's, two of Assmann's, and one of Hottinger's. They were placed in a circular group, with the receiving surfaces 1.25 metres above the ground, in a cultivated field. The receiving surfaces varied from 100 to 500 of square c. m., but were in no case exact, and proper allowance was made. The observations were continued for ten months, through the winter, and the monthly totals showed differences reaching in some cases (in winter) as high as 8 or 9 per cent.; in summer it ran as high as 6.1 per cent. This was in part due to actual differences in rainfall, for two of the instruments, exactly alike and only 1.25 metres apart, showed an average difference of one per cent. The causes of the other differences were hard to trace, but the author concluded that the form used in the Prussian service (a Hellmann) was good, though a change permitting the easy cleansing of the receiver was recommended.

THE HIGHEST METEOROLOGICAL STATION.—In the January JOURNAL it was stated that the cabin of M. Vallot on Mont Blanc, at an altitude of about 14,300 feet, was probably the highest meteorological station in the world. A still loftier station was established in Peru, in 1888, by the Harvard College observatory, at Vincocaya, at an elevation of 14,600 feet above the sea, upon the railroad extending from Mollendo, on the Pacific Ocean, to Puno, on Lake Titicaca, in the Andes Mountains, and at a height of 12,500 feet, another observing post. These stations and others were maintained for a limited time by the aid of the Boyden fund to determine if the climatic conditions

were suitable for astronomical work, and were provided generally with simple self-recording instruments, controlled by direct observations. Puno is situated on an elevated plain, but Vincocaya lies in a pass of the highest Andes, and is, therefore, less favorably situated than if on a summit. Observations, thrice daily, of temperature, sunshine, cloud, wind and rainfall, with recording instruments for the first two elements, were continued for more than a year, and it is expected that observations will shortly be resumed according to a more complete system under the advice of Professor Pickering, the director of the Harvard College observatory. A. L. R.

A Geographical Exhibition in Boston.—Through the efforts of Professor W. M. Davis, of Harvard University, the collection of geographical appliances belonging to the Brooklyn Institute was brought to Boston, where, during the last half of May, it was open gratis to the public under the auspices of the Appalachian Mountain club. The attendance, including many teachers with their classes from Boston and vicinity, numbered over ten thousand. This collection includes all varieties of geographical appliances, chiefly for educational purposes, such as wall-maps, atlases, globes, models, views, diagrams, textbooks, etc., and is comparable to the collection made by the Royal Geographical Society, which was exhibited in London a few years ago.

In the collection of general wall maps those published by D. Reimer, of Berlin, were especially noticeable. They included physical maps of the well known cartographers, H. and R. Kiepert, and the climatological and orographical maps of Bamberg, which showed well the dependence of climate on the geographical conditions of latitude, height and direction of mountain chains, etc.

E. Debes's physical map of the world, on Mercator's projection, gave the best representation of the ocean currents and the limits of pack and drift ice.

Among the models was a curious one of the Atlantic Ocean by A. W. Clayden, of London, in which real water was used with lycopodium powder scattered over the surface. By means of a series of pipes delivering air from a bellows upon the water a system of currents is set up closely resembling the oceanic currents.

Beside a number of geological models from the geographic

laboratory of Harvard College, there were a number of relief maps, including small, immersible reliefs in plaster intended to demonstrate the use of contour maps in the construction of hypsometric maps.

Of more special interest to meteorologists was a series of wall-charts by Dr. R. Hornberger, of Munich, intended to teach the elements of meteorology and climatology, which were inexpensive and well suited to their purpose. These included isothermal and isobaric charts, charts of wind and weather, curves representing the diurnal and annual periods of the meteorological elements.

In the interesting exhibit of the Appalachian Mountain club was a series of Alpine photographs with very fine cloud effects. Perhaps the most remarkable which have been seen in this country were some bromide prints by Mr. Eccles, of the English Alpine Club, representing the mists and clouds about the Matterhorn. Other fine cloud and snow effects were shown in the English photographs of Donkin and in the Italian ones of Sella, taken in the Alps and Caucasus mountains. A. L. R.

Rapid Advance of Clouds.—On Saturday morning, December 7, 1889, there was a remarkably rapid advance of the clouds from west to east over Ann Arbor. At 7 h. 40 m. (central standard time) there was a low bank of clouds in the west, barely visible, but so straight that by perspective it appeared to curve from the north through the west to the south. By 8 h. 5 m. it had covered the whole sky and was closing down on the eastern horizon. The advancing edge was quite smooth and was like the edge of a fold of cotton-cloth. The cloud was a well-defined stratus, rather thin, and the lower surface was even. I did not think of it as a matter of interest until I saw the cloud shutting down in the east. The time of appearance in the west is, therefore, a little uncertain, but as it was fixed by the stages of morning duties and the ringing of bells in this academic town, it was certainly after 7 h. 35 m. (8 o'clock local time) when I first noted it as a very low bank barely above the horizon. The uncertainty at the beginning was less than five minutes and at the end it was much less. The cloud had covered Ann Arbor completely in less than half an hour. The weather for the preceding twenty-four hours had been beautifully clear, and at 7 h. 5 m. there was not a cloud in the sky.

The exact height of this cloud layer is not known, but it ran rather low. From its appearance I would estimate it to have been about two thousand feet high. The dew-point must have been at that time about 3° lower than the surface temperature. At 6 h. 35 m. it was 27° (air temperature 30°.2): at two p. m. it was 42°.5 (air temperature 44°.5). Taking the usual rule that the lower surface of cloud is apt to be one hundred and fifty-three feet above the surface of the earth for every degree difference between the dew point and the air temperature below, the cloud was five hundred and forty-nine feet above the earth. Taking into account the horizon at the observatory, which is unusually clear for an inland station, and the height at the horizon when first seen, the distance from horizon to horizon at the height of a tenth of a mile is over fifty miles, and at the height of half a mile it is about one hundred and twenty miles. The clouds traversed this distance in less than half an hour and must, therefore, have had a speed of over one hundred, and perhaps of over two hundred miles per hour. As a velocity this is extraordinary, and winds of this velocity have only been found in tornadoes and were here not observed, only estimated by the work done.

The meteorological condition at seven o'clock, as appears from the Signal Service synoptic chart for that morning, was as follows: Shallow area of low pressure lay over Ontario and Quebec with its center well to the north of Ottawa. A moderate high-area stood over the southeastern states, extending as far north as West Virginia and Maryland. Between them was an area of clear weather covering the three lower lakes. It was the western edge of this clear area that advanced over Ann Arbor at the time mentioned. At seven o'clock, forty minutes before their appearance the cloud covered the territory from Toledo to Grand Haven and Green Bay, and westward. Manistee, Alpena, Port Huron, and Detroit were clear.

It is highly improbable that the air advanced at any such a speed as that of the advancing edge of cloud. The wind at the surface was westerly to southwesterly, and had a rate of only eight miles per hour at thirty feet above the ground, and there were no signs of high velocity at the height of the clouds. It was undoubtedly only the conditions necessary for the formation of cloud that advanced so rapidly, and these probably because of the favorable diurnal change. The sun had been up about an hour; the soil, which was nearly bare, had begun to warm up,

the temperature was about 32°, and the cloud-area, which was advancing at a leisurely pace (perhaps twenty-five miles) from the west, found for a time, during the rapid increase of morning temperature, the conditions favorable for a more rapid advance, and these conditions were at once utilized. M. W. H.

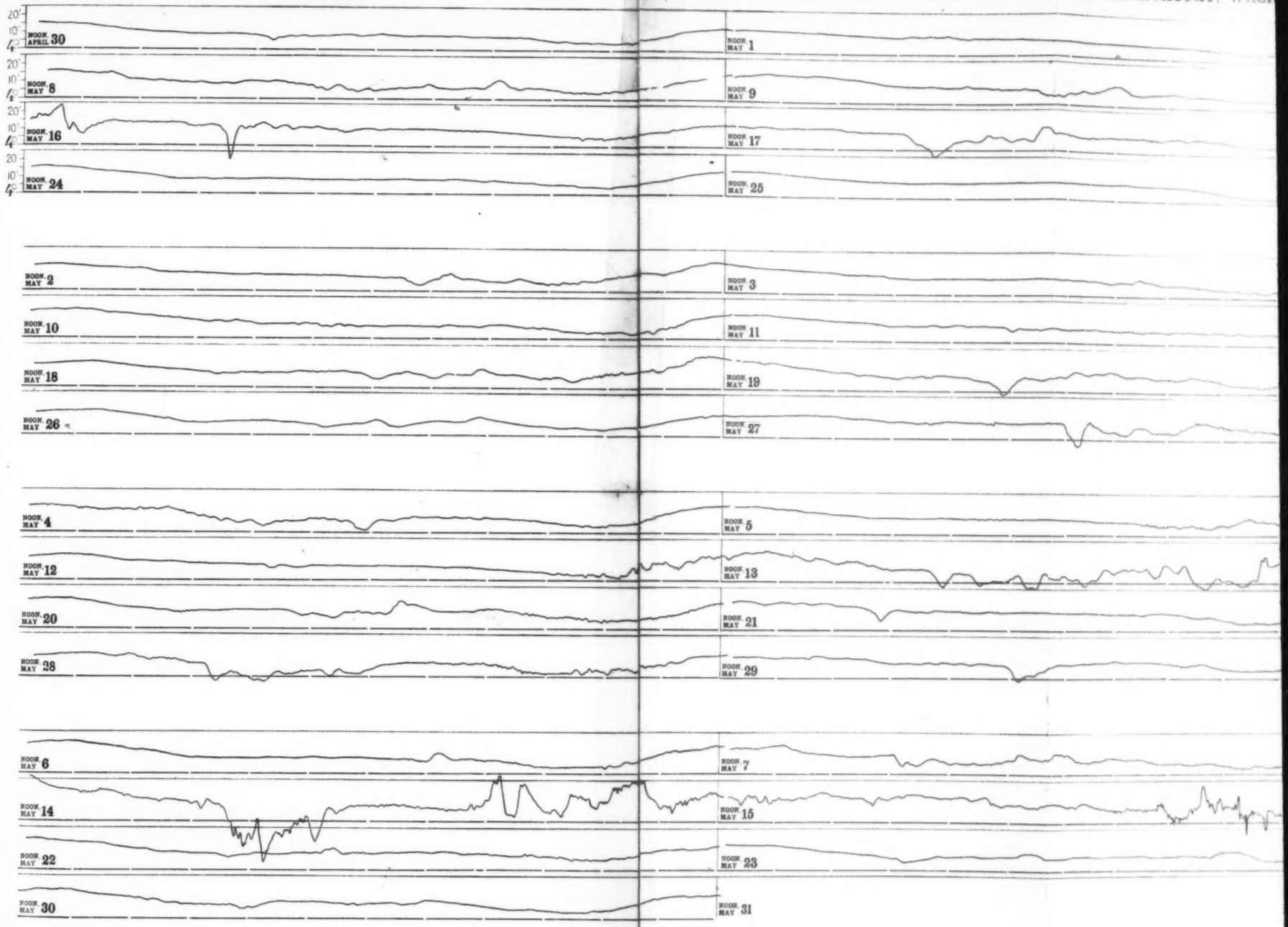
GRAPHIC DAILY RECORD OF THE MAGNETIC DECLINATION, OR VARIATION OF THE COMPASS, AT WASHINGTON.—These curves give a continuous graphic record of the true direction in which the magnetic needle at the Naval Observatory pointed during the past month. The base line shows a slight break at the end of each two hours, 75th meridian time, and the amount of westerly variation at any time is 4° plus the number of minutes represented by the height of the curve above the base line at that time, measured by the scale at the right or left margin of the diagram. The slight breaks in the curve itself occur when the chronograph sheets were changed.

It is of especial interest to study the above curve during the exceptionally severe magnetic storm that occurred about the middle of May. It will be seen that it commenced at 7 A. M. on the 13th, the magnetic disturbance attaining its maximum between 6 and 11 P. M. of the following day, and again between 4 and 10 A. M. of the 15th, not finally ending until the 18th. During this storm the direction of the magnetic needle changed 48′ in 9½ hours. A correspondingly large disturbance was indicated by the instruments registering the horizontal-force component of the earth's magnetism. A marked feature of the storm was an oscillating movement of the north end of the magnetic needle to the eastward, attaining a maximum departure from normal of 35′ on the 14th, between 6 and 11 P. M., accompanied by a large decrease in the horizontal-force and increase in the vertical component. During the afternoon and evening of the 15th the north end of the needle was deflected to the westward, accompanied by a decrease of horizontal force even greater than during the evening of the 14th, and a corresponding decrease in vertical force.

A comparison of the curve during the time of this great magnetic storm with the records of other magnetic observatories in various parts of the world will be of great interest, and observations of auroras, or northern lights, observed during this time will be of more than ordinary importance.—*From the "Pilot Chart."*

(From the Pilot Chart of the North Atlantic Ocean, June, 1891. Hydrographic Office, Navy Department, Washington, D. C.)

GRAPHIC DAILY RECORD OF THE MAGNETIC DECLINATION OR VARIATION OF THE COMPASS, AT THE U. S. NAVAL OBSERVATORY, WASH



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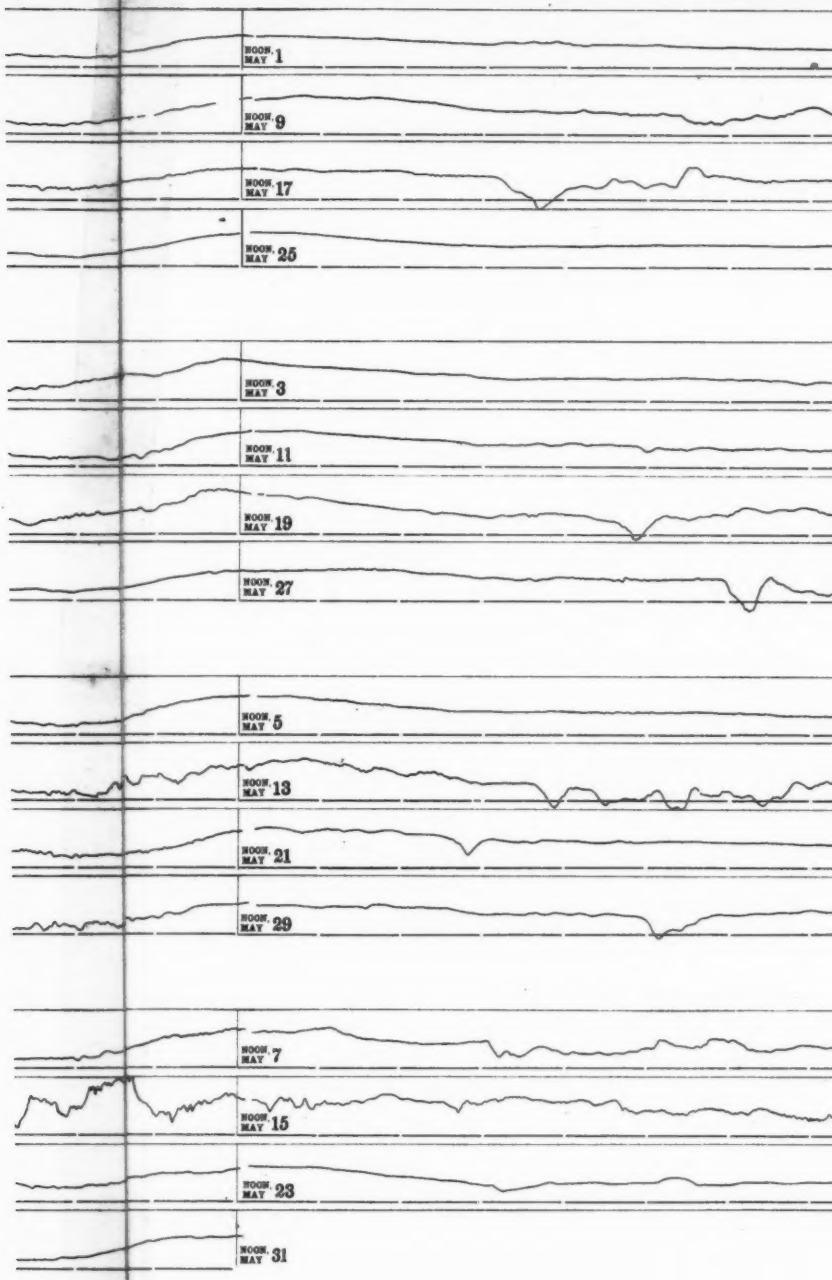

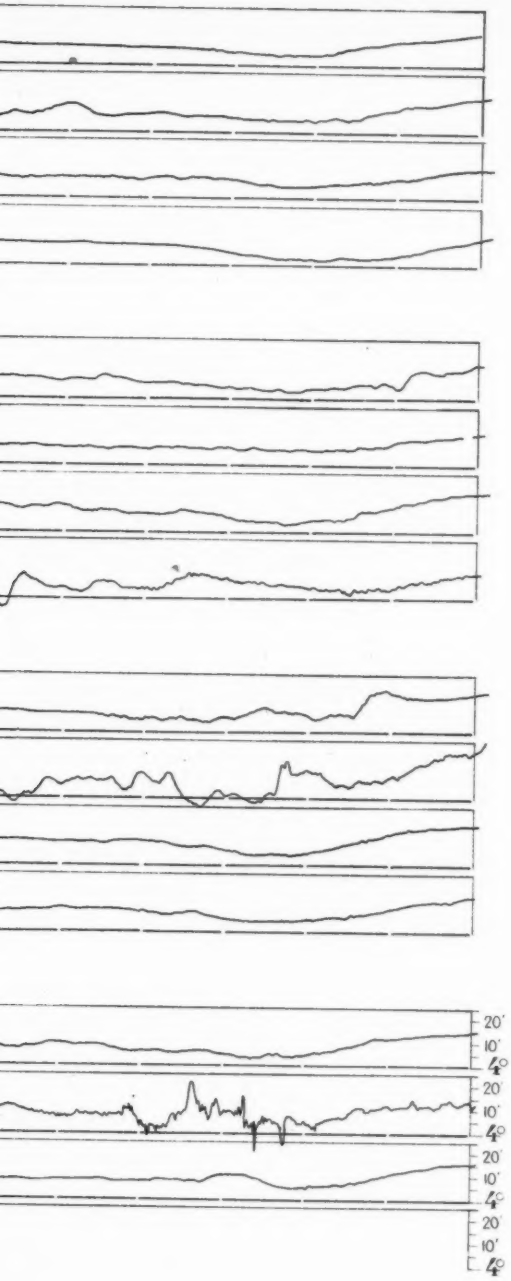
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